

Cognitive Neuroscience from a Behavioral Perspective: A Critique of Chasing Ghosts with Geiger Counters

Steven F. Faux
Drake University

Cognitive neuroscience is a growing new discipline concerned with relating complex behavior to neuroanatomy. Relatively new advances in the imaging of brain function, such as positron emission tomography (PET), have generated hundreds of studies that have demonstrated a number of interesting but also potentially problematic brain–behavior relations. For example, cognitive neuroscientists largely favor interpretations of their data that rely on unobserved hypothetical mechanisms. Their reports often contain phraseology such as *central executive*, *willed action*, and *mental imagery*. As B. F. Skinner argued for decades, cognitive constructs of neurological data may yield nothing more than a conceptual nervous system.

Key words: cognitive neuroscience, PET, fMRI, ERP, brain imaging

During the past 50 years, cognitive scientists have transformed their field by embracing a variety of different disciplines and subdisciplines (Gardner, 1985; Solso, 2001). For example, they have made use of various versions of linguistics (e.g., Chomsky, 1959), philosophy (e.g., Fodor, 1975), symbolic logic (e.g., Newell & Simon, 1976), and connectionist architectures (e.g., Grossberg, 1988). This paper is a critique of certain practices that characterize a popular new version of cognitive science called cognitive neuroscience. Its growing popularity is due, in part, to its use of sophisticated brain-imaging technology involving positron emissions, magnetic resonance, and brain-electric fields. However, cognitive neuroscience has not improved upon the assumptions found in older cognitive sciences. It often assumes that reasonable inferences about unobserved neural mechanisms can be made from overt behavior (see Uttal,

2001). Further, many in cognitive neuroscience attempt to give a brain location to those unobserved processes using gross measures of physiology (e.g., Roland, 1993).

This paper will update an argument originally made by Skinner (e.g., 1938/1991, 1950, 1953, 1974) that superimposing unobserved mechanisms upon the brain results in little more than a “conceptual nervous system,” with a great potential to misguide (see also Uttal, 2001). Much of cognitive neuroscience still relies on mentalistic forms of explanation that either explicitly or implicitly appeal to an inner agent, “the ghost in the machine,” in the words of Ryle (1949). Although the technology of cognitive neuroscience is impressive (e.g., tracking gamma radiation in the brain), it is the opinion of this author that those measurements amount to little more than chasing ghosts with Geiger counters. This paper argues that much of cognitive neuroscience is mere conceptual neurology that tends to obscure rather than further scientific progress. It will also address a number of methodological concerns.

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Address correspondence to Steven F. Faux, Department of Psychology, Drake University, Des Moines, Iowa 50311 (e-mail: steven.faux@drake.edu).

A BRIEF HISTORY OF COGNITIVE NEUROSCIENCE

Cognitive neuroscience emerged as a new discipline when cognitive sci-

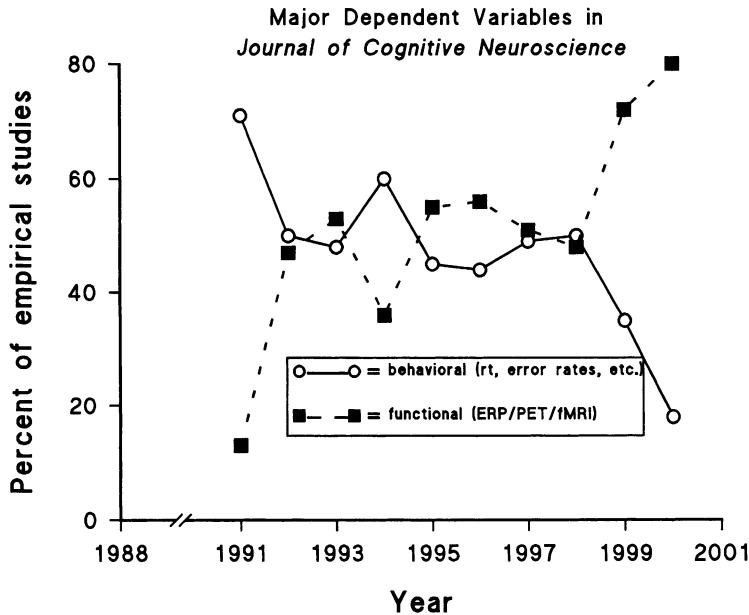


Figure 1. Percentage of empirical articles in each volume of the *Journal of Cognitive Neuroscience* using either traditional behavioral measures (e.g., reaction time, error rates, etc.) or brain-imaging measures (PET, fMRI, or ERP) in the past 10 years.

entists began combining their old cognitive-perceptual paradigms with the new brain-imaging procedures (Servos, 2000). The most important brain-imaging technologies that have shaped the field are positron emission scanning (PET), functional magnetic resonance imaging (fMRI), and event-related potentials (ERPs). These technologies (described in more detail below) give some measure of brain activity with varying degrees of temporal and spatial resolution.

In 1988, the *Journal of Cognitive Neuroscience* began publication, and shortly thereafter, the Cognitive Neuroscience Society (with the acronym CNS) was founded in 1994. CNS began with a membership of about 400 and is now about 2,000 and growing (Tara Miller, personal communication, 2001). The *Journal of Cognitive Neuroscience* is a peer-reviewed quarterly. Its empirical articles are on standard topics of cognitive science, which include working memory, verbal performance, visual perception, and mental imagery. The journal has grown steadily, publishing about 300 pages per year

at its inception and by its 12th volume publishing well over 1,000 pages per year. Citations of cognitive neuroscience studies now appear widely in common textbooks of undergraduate cognitive psychology (e.g., Solso, 2001; Willingham, 2001) and introductory psychology (Kosslyn & Rosenberg, 2001; Myers, 2001).

In the past, cognitive science has relied largely upon reaction time as its primary dependent variable. Reaction times were used as an indirect measure of mental chronometry, that is, the speed of mental processes (e.g., Posner, 1986). Although reaction time is still important, cognitive neuroscience has added three brain-imaging technologies (PET, fMRI, and ERP) to its armament. Tabulation of dependent variables used in all empirical articles in the *Journal of Cognitive Neuroscience* between 1991 and 2000 (see Figure 1) shows an increasing reliance on brain-imaging techniques relative to traditional behavioral measures in cognitive psychology. Cognitive neuroscience has become a brain-imaging science.

It is easy to understand why some

scientists, editors, and readers would be attracted to this imaging technology. The imaging instruments themselves are the result of advances in atomic physics, electromagnetism, and microcomputers. Images from functional brain scans are often color coded (like weather maps) and produce spectacular color plates in journals. The data appear to take the reader a step closer to the "black box" of brain operations. Hence, a brief description of the three most widely used imaging techniques is useful and is given below.

THE IMAGING TECHNOLOGY AND DEPENDENT VARIABLES

Positron Emission Tomography

This technique is essentially a computerized Geiger counter that can identify regions of radioactivity in the brain. Human participants are administered (by inhaling or by injection) radioactive isotopes, such as oxygen-15, nitrogen-13, or carbon-11, that can be absorbed by the brain. Brain regions presumably activated during a cognitive task selectively absorb the isotopes and become differentially radioactive for a short time (see Cabeza & Nyberg, 1997, 2000; Roland, 1993).

Depending upon the protocol, PET can measure regional variations in blood flow, blood volume, oxygen consumption, and glucose utilization, among others (Raichle, 1996, p. 190). Oxygen-15-labeled water with a half-life of 123 s is the substance most often used in PET studies (G. I. Shulman et al., 1997, p. 642). As neurons become active, the labeled water is locally consumed during oxidative metabolism, creating differential regions of radioactivity in the brain (Roland, 1993).

PET scan measures of gamma radiation are converted to estimates of regional cerebral blood flow. The typical scan takes about 40 to 70 s, with a spatial resolution of between 6 and 15 mm. This means that a single PET image represents a summary of brain activity over a period of about 1 min, and that the error in localizing brain activ-

ity may be several millimeters in any direction. PET brain activity is often measured while the participant continuously engages in some behavioral task (e.g., reading a list of concrete nouns presented one at a time or listening for a target tone that requires a button press). Multiple stimuli (trials) usually are presented over the time it takes to make a single PET scan. Because of the temporal constraints, there is little or no ability to resolve PET activity as a function of trials or stimulus presentations within a task. A typical experiment requires six to ten scans made about 10 min apart (Cabeza & Nyberg, 1997, p. 3). Scans from an individual participant are averaged, and then data from multiple participants are grouped to create grand averages.

Functional Magnetic Resonance Imaging

Functional MRI is similar to PET in that it provides a measure of blood flow—in this case, localized blood oxygen (see David, Blamire, & Breiter, 1994, for introductory review). However, it differs in that no radioactive substances are involved. This technique utilizes magnetic fields (having a magnetic strength of about 2 to 4 Tesla) and the magnetic properties of hemoglobin to produce functional brain images. Because deoxyhemoglobin is more magnetic than oxygenated hemoglobin and surrounding tissue, the fMRI machine is able to detect differences in oxygen levels. This technique is called blood-oxygen-level-dependent (BOLD) fMRI.

Although fMRI images can be made quickly, it takes 3 to 6 s for oxygen to concentrate in brain regions that become neurologically active (Servos, 2000), creating a functional limit to scanning rate. An fMRI machine usually scans about every 2 s with a resolution of about 2 mm. Special protocols sometimes allow multiple scans per second. These specifications are significant improvements over PET. However, both fMRI and PET use ex-

perimental methods that are nearly identical.

Event-Related Potentials

ERPs are averaged brain electrical responses (EEG) recorded from the scalp and time locked to specific stimulus presentations (see Coles, Gratton, & Fabiani, 1990, for introductory review). Typically, the recording epoch (i.e., duration) is about 1 s beginning with the onset of the stimulus. ERPs, also called *averaged evoked potentials*, are the average brain response of somewhere between 30 and 1,000 stimulus presentations depending on the signal-to-noise ratio of the background EEG.

ERPs demonstrate that brief stimulus presentations evoke morphologically specific electrical responses from the brain. Early potentials, within the first 200 ms, are thought to reflect the neural volley passing from synaptic junction to synaptic junction. The neural sources of later evoked responses beyond 200 ms, termed *long-latency potentials*, are less easily identified. The tendency in the literature has been to link early ERP responses to primitive sensory systems and long-latency ERPs to complex cognitive-perceptual systems. The functional role of the ERP in the brain, if any, is not known. The peaks and valleys of any given brain potential probably have multiple neural generators with complex interactions that are nearly impossible to tease apart (see Coles et al., 1990, chap. 13; Donchin & Coles, 1988). The most prominent long-latency potentials in the cognitive neuroscience literature are the P300 produced by task-relevant discriminative stimuli (reviewed in Donchin & Coles, 1988), and the N400, produced by contextually irrelevant verbal stimuli (reviewed in Kutas & Van Petten, 1988).

The remainder of the paper will concentrate on PET findings. However, with few exceptions, the problems raised for PET apply in some form to fMRI and ERP.

A PROTOTYPICAL PET STUDY WITH A BRIEF CRITIQUE

To illustrate problems of interpretation and method, a particular PET scan study (Mellet, Tzourio, Denis, & Mazoyer, 1995) will be described in detail. This study has been chosen because it is fairly typical of the experiments in this field. Its results are cited in respected textbooks (e.g., Richardson, 1999, not only cites this study in detail but uses color plates from this study on its cover), and it is one of the rare reports that actually provides some individual-participant data, an issue important to behavior analysis.

Eight individuals participated in three behavioral conditions; PET measurements were made for an 80-s duration in each condition. The first condition, a baseline, consisted of making a PET scan while the volunteer was relaxed with eyes closed. The second, a perception condition, consisted of taking a scan while the participant viewed a cartoon map of an island with various landmarks along its perimeter (huts, trees, wells, mountains, etc.). Participants were instructed to move their eyes systematically clockwise from landmark to landmark on the island, and then to move eyes counterclockwise. The third condition involved mental imagery. With eyes closed as in the baseline condition, the participants were scanned while they mentally visualized the map and moved their eyes as if viewing the landmarks of the map.

From previous work (i.e., Kosslyn et al., 1993), Mellet et al. (1995) expected to find activation of the primary and association visual cortices in the imagery condition relative to baseline. Mellet et al. presented regional cerebral blood flow (rCBF) results for all 8 individuals from six brain regions in both left and right hemispheres (see the histograms in Figure 4 of their report). Positive rCBF values indicated brain activation (increased blood flow), and negative values indicated deactivation (decreased blood flow) relative to base-

line. In the “perception minus baseline” data, activation of primary visual cortex and superior occipital cortex was observed to be positive, relatively large, and consistent across all 8 participants. In addition, relatively consistent positive responses were found in superior parietal and precuneus areas. So far, the interpretation is clear—looking at something activates visual areas of the brain. Less well explained in the Mellet et al. report is that consistent deactivation was observed in superior temporal cortex and the inferior frontal region. A consistent problem with any cognitive interpretation of PET data is determining why certain brain regions are activated while others are deactivated. This issue will be elaborated below (see Additional Problems with PET).

In the “imagery minus baseline” data, what was most striking was the strong variability across participants. The primary visual cortex showed rCBF values with large fluctuations going in both positive and negative directions depending on the participant. The superior occipital cortex data showed a little more consistency but with very weak values generally in the direction of activation (increased blood flow). All other brain regions showed fairly strong variability.

Despite variability among participants, Mellet et al. (1995) should be commended for publishing a full-color plate of a PET scan identified as “Subject 8” (see their Figure 5). Systematic presentation of individual data is rarely done in the field. This brain scan showed the superior occipital cortex lighting up with hot-colored values (reds and yellows in the original plate) in the mental imaging condition, indicating that rCBF values were the largest in this region relative to all other brain regions. (This is the result that Mellet et al. had predicted.) Unfortunately, these results do not represent the full story. Such color-coded data have the potential to mislead unless carefully analyzed. Colors on a PET scan are similar to the colors on a tem-

perature map. However, on a PET brain image, hot colors (reds and yellows) represent regions of relatively high radiation and cool colors (greens and blues) represent regions of relatively low radiation. Although the Mellet et al. PET scan graph of Subject 8 in the imaging condition showed the existence of many red hot areas in superior occipital cortex, the same data presented in histogram form for that participant reveal that actually very small rCBF values were coded as red hot. In fact, little rCBF variation was observed in other brain regions for this participant, suggesting the color scale (going from hot red to cool blue) represented an extremely small range of numeric values. (Perceptually significant color differences in PET scan graphs do not necessarily equal physiologically important differences.) For most other participants the rCBF values in superior occipital cortex (as indicated by histogram) were dwarfed by values found in other brain regions. Thus, if all participant data were presented in color image form, no consistent patterns of anatomical activation would be evident. In other words, the red hot regions would be quite different for each individual. This lack of consistency across individuals blurs any useful scientific interpretation.

Despite large individual variability, the authors concluded that mental imagery is associated with activation of the superior occipital cortex. Mellet et al. (1995) used their data to support the idea that the visual association cortex creates mental images or “stored representations common to visual perception, and a similarity of processes that access and manipulate these representations” (p. 433). In fact, the data and the conclusions are not terribly persuasive, because the behavior of mental imaging appears to produce no consistent topographic relations across individuals.

Linking mental imagery with PET is not just a process inferred, but is a process “deferred.” As Skinner (1974) argued, “An inner copy makes no prog-

ress whatsoever in explaining . . . [the] physiology of perception" (p. 81). He goes on to say, "It is as difficult to explain how we see a picture in the occipital cortex of the brain as to explain how we see the outside world, which it is said to represent" (p. 81). Map reading is not an ability that needs to be conferred on neurons. The ghost is not in the machine. It is just a machine.

This paper is not intended to be a general statement against the study of brain-behavior relations. Instead, this is a proposal that science progresses best when physical brain measurements are tied to overt behaviors. As Skinner (1938/1991) stated, "Before . . . [a neurological] fact may be shown to account for a fact of behavior, both must be quantitatively described and shown to correspond in all their properties" (p. 422). Mental imaging as an inferred behavior has not been adequately quantified to meet the above criterion.

EXPERIMENTAL DESIGN: THE SUBTRACTION METHOD

This critique of practices within cognitive neuroscience requires some understanding of the most widely used experimental design in the field: the subtraction procedure. It is the standard design of PET and fMRI studies (Raichle, 1996), and variants of the design are often found in ERP studies. The subtraction method, involves the following steps (elaborating on Smith, 1997):

1. Identify some treatment task that involves the cognitive process, *P*.
2. Identify some baseline task that is identical to the treatment task but does not involve cognitive process, *P*.
3. Collect separate brain scans during the baseline and treatment tasks. Repeat scanning several times within subjects and tasks. Compute an average scan for each individual within each task.
4. Subtract average baseline scan results from average treatment scan results. Compute a grand average across

subjects of all scans. Find brain regions with averages that are statistically different from zero.

5. Conclude that statistically significant brain regions account for cognitive process, *P*.

Three main critiques of the subtraction method are provided, and each revolves around the cognitive constructs themselves.

Criticism 1: Cognitive Atoms Have Not Been Identified

Because any given cognitive process is based on inference, it seems impossible that a treatment task could ever be designed so that it differs from a baseline task by only a single brain operation (cf. Sargent, 1994). To their credit, cognitive psychologists have recognized this issue and have even given it a name—the *pure insertion problem* (Sartori & Umiltà, 2000). As an example, a recent cognitive neuroscience study of reading comprehension notes, "Even simple tasks, hypothesized to index selectively particular aspects of language processing, often do not tap only one component of language processing but encompass a complex chain of processing" (Bavelier et al., 1997, p. 666). This statement does not go far enough. The claim that a given behavior differs from another behavior by a *single* cognitive process is nothing more than a best guess. The word *single* is an uninformative modifier of an inherently ill-defined concept. Because cognitive operations cannot be directly observed, subcomponents of an operation cannot be dissected or disentangled, let alone counted.

Cognitive neuroscientists often make arguments as if everyone has accepted a periodic table of cognitive atoms known to make up cognitive compounds. For example, Smith (1997) argues that "you can end up drawing the wrong conclusion, if the concept you are attempting to localize [in the brain] is itself readily decomposable into more primitive cognitive concepts" (p.

168). It is difficult to conceptualize a primitive cognitive construct, particularly because no criteria are given. This problem of primitivism is made evident by Smith's own attempt to explain unexpected results in a spatial working memory task. He states, "Spatial working memory can be decomposed into a pure storage component (a spatial buffer) and a rehearsal component, . . . the latter involv[ing] selective attention" (p. 168). First, it is not likely that there would be much agreement on this particular decomposition of spatial working memory (cf. Johnston & Dark, 1986). Second, one must wonder how useful it is to break one vague construct into three vague constructs. In a sense, however, Smith is correct. If relevant variables are not under proper experimental control, then the results are likely to be uninformative or misleading.

Criticism 2: Vague Cognitive Labels Do Not Elucidate Vague Anatomy

Cognitive neuroscientists have failed to justify why unobserved cognitive constructs make useful labels for particular brain regions. PET and fMRI reports assume that different images between the treatment task and baseline task reveal anatomical locations important to the production of some *cognition*. However, it is more likely that PET or fMRI changes exist, not because a single cognitive process has been segregated, but because treatment and baseline simply involve different *behaviors*.

Uttal (2001), from another behavioral perspective, has described cognitive neuroscience as the "new phrenology." At best, PET and fMRI measurements take us from not knowing what is happening in the whole brain to not knowing what is happening in some particular gyrus. It is tempting to counter that "this is progress," but there are reasons to believe that this form of brain reductionism is inappropriate. For example, brain-imaging procedures are sensitive only to large regional

changes in activation, involving perhaps millions of neurons. Brain scans can and do miss smaller regions of activation (e.g., Fitzpatrick & Rothman, 1999, review some current controversies in neuroenergetics). Neurological significance is not necessarily that which is "large." Further, PET and fMRI are not direct measures of neural activity, only blood flow. It is an assumption that momentary regional blood flow (a slow process of several seconds after a stimulus; see Servos, 2000) reflects the most relevant neural regions of a behavior.

It is a little frightening when one strings together the assumptions made in PET studies. PET investigators assume that increased gamma radiation indexes increased cerebral blood flow, which presumably indexes neural activity, which presumably indexes cognitive processing. Skinner (1950) might as well have been addressing PET assumptions when he criticized mental and physiological theories for being based on "events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (p. 193).

Servos (2000) argues that cognitive cataloging of cortical anatomy is an important scientific step, even if it is not much different than "stamp collecting" (p. 72). However, there are reasons to disagree. In cognitive neuroscience the cognitive events to be classified are obscure, unlike stamps. And, although brain regions can be observed and defined in gross terms, the underlying circuits that mediate cognition cannot. It is doubtful that science can progress by making gross physiological measurements to support ill-defined cognitive constructs.

Level of resolution is still not the root of the problem. Use of cognitive metaphors cause miscommunication even when studies are conducted at the cellular level. For example, Shadlen and Newsome (1996) studied the firing of nerve cells in the lateral intraparietal cortex and showed that some cell ac-

tivity was correlated with the difficulty of perceptual decisions. Such cells were coined *decision cells*. Provoked by this terminology, a news feature in a recent issue of *Nature* concluded "Decision cells could form the basis for a 'central executive' in the brain" (Kast, 2001, p. 127). The effectiveness of a science must be judged by the precision of its terminology.

Criticism 3: The "Cognitive" in Cognitive Neuroscience Is Not Tested

In the subtraction method, as Smith (1997) has recognized, "no matter what appears in the [treatment minus baseline] difference image" (p. 167), some proposed cognitive processes can be said to be responsible for that difference. Thus, cognitive constructs are not directly tested in the subtraction method, because no brain-imaging result could ever refute a cognitive theory. Instead, cognitive constructs are only "mapped." True, brain maps may be revised from study to study, but the cognitive constructs themselves have a status that is relatively immune to experimental revision.

The correlation of so-called cognition with brain activity is inevitable. Why inevitable? Because for any given behavior, *X*, there will always exist some unobserved cognitive explanation, *Y*. Once *X* is legitimately tied to a brain measurement, there is little to hinder *Y* from being tied to the brain as well.

In the whole enterprise, there is no indication of how one can go from brain maps to controlling or manipulating behavioral or neurological variables. The goal of the research program appears to be to map and label the brain. One cannot object to mapping or even labeling parts of the brain as being associated with specific behavioral functions. However, there is no good reason to make cognitive terminology the de facto language of the neuroscience of complex behavior.

**HOMUNCULUS RESURRECTED:
THE CENTRAL EXECUTIVE**

Dennett (1991) has argued that a pervasive flaw of cognitive neuroscience models is that they "still presuppose that somewhere, conveniently hidden in the obscure 'center' of the mind/brain, there is a Cartesian Theater, a place where 'it all comes together' and consciousness happens" (p. 39). Sadly, one does not have to look far to find a neurological discussion of the "central executive" (e.g., Baddeley, 1995; Collette et al., 1999), "willed action" (e.g., Badgaiyan, 2000; Frith, Friston, Liddle, & Frackowiak, 1991), or "supervisory attentional systems" (e.g., Bayliss & Roodenrys, 2000; Shallice, 1991). In a recent review article published in *Psychological Bulletin* and titled "Cognitive Neuroscience: Origins and Promise," Posner and DiGirolamo (2000) state, "We believe . . . attention and volition are at the very heart of the psychological enterprise" (p. 885). Yet, attention and volition are vague concepts that could be inferred from almost any behavior other than sleeping. How can such broad concepts be at the heart of a scientific enterprise? Scientific terminology without behavioral precision is at risk of being useless reification.

One of the most popular metaphors of cognitive neuroscience is the central executive, a mechanism that is often viewed as being independent of the senses, providing output without input (see, e.g., Kast, 2001). The cognitive literature often locates the central executive in the frontal lobe (Gazzaniga, Ivry, & Mangun, 1998). For example, Marshuetz, Smith, Jonides, DeGutis, and Chenevert (2000) cite literature to argue that "the ventrolateral and dorsolateral prefrontal cortex . . . are thought to mediate executive processes" (p. 130). They then define the executive processes. Again, to quote Marshuetz et al. (2000): "Among [the executive processes] are attention-and-inhibition . . . , task management . . . ,

monitoring . . . , and coding . . . in memory” (p. 131). How can neuro-anatomy clarify the issue of task management, coding, and memory as executive processes? How would one recognize the operations of an executive in terms of physiology? These questions are seldom addressed. A behavioral perspective suggests that legitimate brain-behavior relations become trivialized when tied to a metaphorical central executive.

The problem of vague terminology is illustrated in a widely cited PET study of willed action, considered by some cognitive neuroscientists to be a classic in the field (e.g., Gazzaniga et al., 1998; R. G. Shulman, 1996). The report is by Frith et al. (1991) titled “Willed Action and the Prefrontal Cortex in Man.” Six volunteers in each of two studies inhaled oxygen-15-labeled carbon dioxide while they performed simple tasks for 3 min. In one study the tasks were verbal in nature, and in the other the tasks were somatosensory. The baseline task in the verbal study consisted of repeating common words presented by the experimenter, such as *man* or *hot*. The treatment task required the volunteers to generate words beginning with the letter F, such as *fox* or *first*, as the experimenter prompted each response by saying “next.” The baseline task in the somatosensory condition consisted of volunteers moving the same finger touched randomly by the experimenter. The treatment task consisted of moving any finger of choice (avoiding repetition) when prompted by a touch on a finger.

According to Frith et al. (1991) both baseline tasks involved volunteers giving an exact response to a stimulus, whereas both treatment tasks involved making a choice from a population of potential responses. By their interpretation, the primary differences between the baseline and treatment tasks were *willed action* or *internal response generation*—defined as novel behaviors not specified by a stimulus. In both modalities, verbal and somatosensory,

Frith et al. found that the treatment condition minus the baseline condition showed increased activity in the dorsolateral prefrontal cortex with some involvement of the anterior cingulate cortex. Their primary interpretation was that the frontal cortex has a significant role in willed action.

One must wonder what behaviors would and would not be considered “willed actions.” It is hard to imagine reasonable definitional criteria. A lack of stimulus control in an experiment does not result in “willed” behaviors; it merely results in behaviors not under the control of the experimenter. Frith et al. (1991) are probably not suggesting that complex behaviors are unlawful, but if they are not making that suggestion, then a much better terminology is needed.

What kind of empirical data would challenge Frith et al.’s (1991) results and interpretation? R. G. Shulman (1996) struggled with this question after having tried to replicate their results using fMRI. His laboratory found correlations in slightly different brain regions. In a candid and bold interview with the *Journal of Cognitive Neuroscience* in 1996, Shulman argued that the use of cognitive concepts in neuroscience has interfered with understanding brain activity. He argued, moreover, that PET and fMRI data “when examined more critically are not really telling us much about how the brain enables cognition” (p. 475). Referring to constructs such as willed action, attention, and working memory, Shulman said, “Given the primitive state of cognitive concepts and the present strengths of functional imaging this is the time for revising concepts in response to . . . the experiments, rather than asking experiments to subserve the concepts” (p. 476). No behaviorist could supply stronger testimony. Indeed, neuroscience has made considerable strides in tying observable behaviors to specific brain locales (see, e.g., Kandel, Schwartz, & Jessell, 2000, for current reviews). By contrast, there is no strong evidence that in-

ferred mental operations have clarified neurophysiology.

ADDITIONAL PROBLEMS WITH PET

Cognitive constructs are not the only source of ambiguity in PET. The PET datum itself is inherently difficult to interpret.

Distinguishing Activation from Deactivation

The PET concepts of activation and deactivation are sometimes misconstrued as being related to neural excitation and inhibition. In fact, these PET terms have no clear relation to underlying neural behavior. Perhaps without justification, the PET literature has routinely emphasized behavioral correlations with activation over those with deactivation (reviewed by Cabeza & Nyberg, 1997, 2000). However, PET activation and deactivation have little to do with a neuron's firing threshold. Activated and deactivated brain regions are determined statistically relative to some baseline, and reflect changes in blood flow and metabolism. Specifically, brain activation refers to an increase in localized blood flow, and deactivation refers to a decrease. Although excited neurons would typically expend more energy than inhibited ones, neurons that produce inhibitory postsynaptic potentials do expend metabolic resources, and large groups of such neurons could show up as "active" on a PET scan (Kandel et al., 2000; Roland, 1993). Moreover, excited neurons are often closely surrounded by inhibited (hyperpolarized) neurons (Roland, 1993). "Deactivated" neither means inhibited nor behaviorally unimportant. So-called deactivated regions could still have considerable neural activity and therefore could be a neural source for any given behavior. For these reasons, designated regions of PET activation are ambiguous with respect to the behavior of neurons.

Regions of activation or deactivation cannot be predicted with any precision.

With the inherent ambiguities of positive and negative values in PET data, one can appreciate why Frith et al. (1991), summarized above, might puzzle over the following: "In both [our] studies, *decreases* [italics added] were observed in cortical areas concerned with the modalities specific to the study" (p. 245). Specifically, Frith et al., in a word-generation study, found *decreased* activity in the left superior temporal cortex (a language-related region), and, in a somatosensory study, found *decreased* activity in the sensorimotor (pre- and postcentral) cortex. These findings, however, were glossed over because both studies found *activation* in the frontal lobe. Apparently, only the frontal lobes showed enough of the "central executive" characteristic of being "active" at a consistent location "where it all comes together."

The Problem of Intrinsic Variability and Averaging

Attempts to manipulate variability in brain-imaging experiments have not been made at the individual level. Typically, data are grouped and individual variability is obscured. The cognitive neuroscience approach appears to accept that large variation is intrinsic to the operations of the brain, and that experimental control of individual variation is not possible. As Sidman (1960) has argued, "Acceptance of variability as unavoidable or, in some sense, as representative of the 'real world' is a philosophy that leads to the ignoring of relevant factors" (p. 152). Unfortunately, in most PET, fMRI, and ERP studies total variability is mostly swept under the rug. Multiple brain-imaging measurements over time are averaged (a process called signal averaging) within an individual to determine the presence or absence of a neural response. Individual averages are then grouped to create grand averages. Individual results are rarely displayed, and brain maps are never displayed with error bars. Both intraindividual differences and interindividual differ-

ences are obscured (Raichle, 1996, p. 191). With so much variation, it is reasonable to ask how well averages account for individual results. This question has not been addressed.

The Problem of Statistical Tests

This is not the place for a detailed statistical critique. However, in PET and fMRI, thousands of measurements make up a single brain image. Further, a single brain scan will produce multiple brain slices several millimeters apart. Standard multivariate statistics are not possible because there are many more measurements than there are participants. However, some data-reduction techniques have attempted to solve this problem (see, e.g., Almeida & Ledberg, 2002; Fletcher et al., 1996). Typically, studies use univariate statistical tests on each of the thousands of voxels (pixels) in a PET image. Not only does Type I error inflate due to multiple correlated tests, but statistical significance, accurate or not, may have little direct relation to neurological significance.

The Problem of Replication

Given the kinds of problems outlined here, there should be no surprise that replication is a difficult proposition in many brain-imaging studies of cognitive neuroscience. Rare is inter-group replication, quite rare is intra-subject replication, and intersubject replication is extremely rare (see Cabeza & Nyberg, 1997, 2000, for reviews).

The problem of replication cannot be thoroughly reviewed here, but the reader is referred to Cabeza and Nyberg (1997) for an extensive review of 73 PET studies with tabular summaries of each study. Sixteen of those studies were categorized under the topic of attention, even though most studies used very different behavioral tasks. Even so, they concluded that attention “generally engages frontal and parietal cortices” (p. 21). More striking, however, is the variability of findings among

studies even when similar tasks were used. For example, five of the “attention” studies used comparable versions of the famous Stroop task involving color naming (see Table 1 in Cabeza & Nyberg). Totalling over the five studies, 20 regions of brain activation were found to be related to the Stroop task. Two studies showed Stroop-related brain activation predominantly in the left hemisphere, two showed activation predominantly in the right, and one showed bilateral activation. No single region of brain activation was common to all five studies. Three of the studies agreed on the involvement of Brodmann area 32 on the left, and another two agreed on Brodmann area 40 on the left. The striking finding was not the agreement but the disproportionate amount of disagreement (see Tables 1 through 8 in Cabeza & Nyberg). Cabeza and Nyberg admit that PET results are “determined to a high degree by both the particular target and reference tasks employed” (p. 21). Further, vague cognitive constructs tied to flawed experimental designs produce little reliability.

CONCLUSION

Cognitive neuroscience is gaining in popularity because of its attempt to localize traditional cognitive constructs in neuroanatomy. However, too many proposed cognitive mechanisms are vague, unnecessarily complex, premature, and amount to little more than inferred guesswork. Unobservable behaviors of the mind, like volition, central executive function, and mental imagery, do not enhance understanding of empirical brain operations. In fact, such terminology obscures more than clarifies. Significant problems exist with the standard experimental paradigm, the subtraction method. For example, cognitive constructs are not tested in the subtraction method, and brain images are incapable of refuting traditional theories of cognition. Instead, cognitive constructs are being used as labels to name the proposed

functions of the cortex. Cognitive neuroscience theories cannot predict important variations in data, such as when activation and deactivation should be found. Brain maps add little to science if they provide little more than vague terms attached to vague physiology.

Although brain-behavior mapping has legitimate uses, beautiful colors in a PET brain image do not shed light on an unseen intelligence. Some cognitive neuroscientists may insist that there is an executive (ghost) in the machine, but a behavioral analysis of the brain will successfully dispense with such figments in favor of the functional relations of directly observed (and not inferred) variables of behavior and physiology. In sum, cognitive neuroscience has not justified how the study of inferred cognitive constructs can clarify our understanding of brain-behavior relations.

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